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"WHO IS FEEBLE-MINDED?" A REPLY TO MR. KOHS.¹

J. E. WALLACE WALLIN.²

Had Mr. Kohs deferred his criticism of my "Who is Feeble-Minded?" until the complete discussion had appeared in print, and had he given more diligent heed to what I actually said and had he also borne in mind what I did not say or even intimate, he could have avoided, had he so desired, making criticisms based upon assumptions and misconceptions which are not justified by the facts, and likewise avoided indulging in personal innuendos which settle no questions and are, in any case, irrelevant to a scientific consideration of the questions at issue.

May I suggest that scientific discussion will be more profitable if scientific issues are not obfuscated by the introduction of considerations that are wholly irrelevant to the specific facts in controversy and the arguments based upon such alleged facts.

I shall consider seriatim the chief points made in the critique which involve issues of fact.

1. A "particular object of recurrent attack" in my paper was not "Goddard's 1911 revision of the Binet scale." The paper was part of a discussion devoted to a consideration of the standards of determining feeble-mindedness by mental age. The cases examined were rated by Goddard's 1911 scale primarily because it "is usually used in this country for diagnosing feeble-mindedness" and because it is based upon tests of American children. Such criticism as was made of the scale in the section of the chapter that was read was merely incidental. The accuracy of Goddard's version was considered in an earlier section of the chapter. To this I shall return.

2. "Goddard's 1911 revision of the Binet scale" is not "a matter of past history." The 1913 article referred to does not in any sense supersede the scale. It is not a 1913 scale, in fact, it is not a scale at all, but a discussion of the administration of some of the tests in the 1911 scale. Goddard himself specifically states that the

¹See articles by Dr. Wallin and Mr. Kohs in the January and March numbers of this Journal respectively.

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"original pamphlet" * * * still remains as our Vineland adaptation of the Binet Scale." Obviously according to Goddard the scale is not "a matter of past history." I was not unaware, as Mr. Kohs intimates, that Goddard had altered his position in this article with respect to the "fifteen year" and "adult" tests, because they are not "reliable," "and cannot be scored," but that does not affect the accuracy of my statement that the "rule is being constantly thus applied in practice."

We have been told, speaking of the Vineland version, that "between 1910 and 1914 20,000 booklets * * * were printed and distributed from the Vineland Laboratory alone." It is fair to assume that the scale must be used by thousands of testers, the majority of whom, in all probability, are the merest amateurs. How many of these testers have ever seen the 1913 statement and are heeding its suggestions? Probably only those who are regularly reading the *Training School Bulletin* and the serious scientific students of mental testing. But for every scientific student there are probably ten "Binet testers" (as I use the term) using the scale who make little, if any, attempt to follow scientific discussions. Last November I observed a teacher giving the Binet tests at a public demonstration in a Missouri city. She was using Goddard's 1911 scale. She knew nothing whatever of the 1913 statement—parenthetically, she made 7 errors in administering the tests in 20 minutes of testing, and in reply to a question from the audience said that Binet had demonstrated the accuracy of the tests by testing 5,000 normal children. Likewise in a juvenile court I found last year that the results were scored according to Goddard's "original pamphlet." My frequent indictments of the "Binet testers" have not "magnified" a defect "enormously out of proportion."

In using the advance scoring from the two upper years I was merely following a practice which survives, I am sure, among a large number of Binet testers. But, be this as it may, my practice of giving *all* the Binet tests could not possibly work any injustice to the individuals tested, for it is evident that by giving them credit from the higher tests some of them were able to make a higher score than they otherwise could have done. Without these tests some of them would have graded lower than they actually did. This is shown by the following figures which give the "old scores" (1911 pamphlet) and the "new scores" (minus "tests beyond age 12"), the latter of which, according to our critic, render our results "comparable" with "a tool which is being 'suggestions' given are mostly 'supplementary' and that the

applied by others in a much improved form"—by how many others and how "much" improved no one has ever shown:

Subject..... A B C D E F G H I J K L M
Old

Scores.... 10.6 10.6 11.2 11.2 11.4 10.8 10.6 12.8 12.6 13.4 12.4 12.8 12.8

New

Scores.... 10.6 10.6 11.2 11.0 11.2 10.4 10.4 11.8 11.8 11.6 11.4 11.8 11.6

It will be seen that the new scores are exactly the same as the old scores in the case of three subjects, while they are lower for all the others. How about subject F and the 10³ standard of feeble-mindedness here! and how about the 11³ standard of borderlinity which places three of the students at or below the borderline, while three others escape the borderline zone by one bare point! The "improved form" renders everyone of these subjects feeble-minded on the 11 and 12 year standards and four feeble-minded on the 10 year standard. Does our critic maintain that the "improved" score does ampler justice to the mentality of these 13 subjects than the rating which we gave them in the first instance? If he does, we do not accept his conclusions.

Moreover, in condemning my own practice of giving all the Binet tests in order to do the subjects full justice, Mr. Kohs doubly condemns his own procedure in giving the Binet, for he neither follows the 1913 suggestions to *eliminate all* the fifteen-year and adult tests nor the requirement of the 1911 pamphlet to *include all* of them, but arbitrarily selects two of the higher tests for reasons of his own. (The Practicability of the Binet Scale, etc.) Does he, notwithstanding these liberties, intend to imply that, while our results are incomparable, his own results are "comparable" with the work of others?

3. Mr. Kohs' implication, by his citation of irresponsible newspaper reports and his final reference to my "indictment of the Binet scale," that I regard the scale as "utterly unreliable," is neither warranted by anything said by me in the paper which he criticises nor by anything which I have previously said. The fact is, that I have used the scale continuously ever since the Vineland 1908 version appeared, and still use it. If my judgment is "impregnated with prejudice," the prejudice should be in favor of the scale and not against it. But my admiration of the scale, keen as it has been and which I have often expressed, has not made me blind to its defects and limitations. My indictment—to appropriate my critic's term—of the scale is three-fold.

In the first place, the scale is not able to do everything that its ardent devotees would have it do. Kohs assures us that "in order to determine feeble-mindedness supplementary tests to the Binet are only necessary when a subject's mental age falls at 10⁴, 11⁰, 11¹ 11² * * * We find the Binet scale itself entirely satisfactory when we have to determine the existence of mental deficiency in a subject testing 10³ or below, or its non-existence in one testing 11³ or over" (even with a scale limited to age XII!). Goddard, in a statement quoted by Mr. Kohs, affirms that "nothing else is needed in the great mass of cases than this test" (the Binet-Simon). I dissent from these conclusions. In a considerable number of cases the Binet results, or the results from any other tests at present available, do not of themselves afford a sufficient basis for diagnosis. Lest my critic should regard my "strictures" as "unfounded" and due to "prejudice," "ignorance", or "perversity", I shall cite the opinions of other workers, some of whom are among the pioneers in the work and whose scientific competency will not be questioned.

Binet, in keeping with his spirit of cautious inquiry, was extremely modest in respect to the possibilities of his scale: "Our scale of intelligence gives the actual level of an intelligence without analyzing it, and without informing us about the mentality." "It is an error to pretend that the scale alone is sufficient to enable us to know the character of children; in reality it is made to aid observation, to complete and control it, but by no means to replace it."

Binet and Simon further assert that the fact that a "defective of eleven (who) is at the level of a child of nine" * * * * "has no bearing on the cause of the retardation, nor upon its particular nature, nor upon the means for rectifying it." Concerning their standards for the initial selection of suspected cases of mental deficiency, they say: "Let us even say boldly that if, by some unhappy chance, other finer methods should conflict with this, and indicate as defective a child who has shown himself well adapted to school life, it is school life which should be considered the more important test." They would probably apply the same remark to "social life," which, as Witmer insists, constitutes the ultimate test. "We may rigorously exclude from the class of defectives the child who is simply ignorant, but there is a whole series of complex cases intermediate between the ignorant and the defective. * * * * "We hope they (the 'normal cases') will be kept * * * * in the ordinary school." "We hope it, we even demand it with all our power." "All the decisions arrived at"—referring to the examination of suspected school defectives—"are to be regarded as provisional." Concerning

the bearing of heredity, they are emphatically skeptical: "But the above observations upon his heredity do not bear upon the question of whether he is a defective, and throw only the dimmest light upon his character and the manner in which one should treat him."

* * * * "At most the discovery of a pathological heredity might incline one in a doubtful case towards a diagnosis of mental deficiency, but yet one should be extremely cautious about permitting oneself to be influenced in this way, for we are ignorant to a most incredible degree concerning the heredity and antecedents of normal children, and as our ignorance in this respect is so complete we are unable to say precisely what is really pathological in the heredity and the antecedents of those who are abnormal. Information of this kind, therefore, is not directly useful." And yet, Mr. Kohs, speaking *ex cathedra*, tells us that he is able to "clinch the argument" whenever he finds, or thinks that he finds, that the "family was feeble-minded." The fact is, that there is not that degree of agreement about the hereditary factors which our critic assumes, either as to point of fact or point of method. The staff of medical inspectors who are assisting me in the collection of my heredity data and who, in turn, are assisted by the instructional and nursing staff, asked last year to be relieved of the heredity inquiry, because they regarded most of the information as practically useless and little better, in point of reliability, than a guess—we, however, still continue these inquiries.

Witmer writes: "Where we shall draw the line between the child whom we shall call a normal child with mental defects, and a subnormal or feeble-minded who is mentally defective, is a problem which cannot be wholly solved within the realm of psychology. No Binet-Simon tests, nor any other tests, will inform us as to what we shall consider feeble-minded." Witmer flatly rejects the conclusions which Mr. Kohs bids us accept for fear we should "lead the general public astray," or "sacrifice the normal progress of applied psychology"—as though the normal progress of applied psychology cannot face honest criticism.

W. E. Fernald declares that "an accurate and incontestable diagnosis of one of these borderline cases can be satisfactorily made only after a thorough physical examination of the patient, knowledge of the family history, personal history," etc. * * * * "The Binet tests, in the hands of competent examiners, usually corroborate the results of clinical examinations in the recognition of all degrees of mental defect in children under 10 and of pronounced mental defect in older persons. These tests are not so effective in the detec-

tion of slight mental defect in world-wise adolescents and adults. In other words, the Binet tests corroborate where we do not need corroboration, and are not decisive where the differential diagnosis of the high-grade defective from the normal is the question." According to Fernald the tests fail at the very point where confirmation is most needed.

Miller writes: "Those who call in question the value of the result obtained by this method do not claim that the Binet-Simon tests are without merit. They say that they are valuable but not completely adequate. They claim that they should only be used by people who are thoroughly trained in other methods of psychological examination."

Richards says: "Whenever I have considered a child feeble-minded, I have always been mistaken. This is true of most persons who work with defectives * * * * most of them make just as bad mistakes as I do, which is comforting." (Richards gives instances of mistaken diagnoses, similar to those I have given based on Binet diagnoses.) "To come right down to actual facts what do any of us know about feeble-mindedness?" Dr. Richards will take note that Mr. Kohs is able to diagnose feeble-mindedness by the Binet scale alone except between 10⁴ and 11², and that he does not find it necessary to diagnose any cases as "doubtful," "reserved," or "backward."

Also Langmead: "I do not think that we can label a child as defective in mind by a fixed test, or set of tests, no matter how carefully thought out * * * Judged by them alone, the minds of many children who are not mentally defective will be weighed in the balance and found wanting"—and that is exactly the situation that confronts us today.

Huey states: "Such persons (namely, the 'many non-psychologists') will at least find the Binet and other series of tests an enrichment of their stock of homemade devices, often economical of time and giving a glimpse at least, of mental deficiency in more varied and representative directions. Each will make these tests somewhat in his own fashion, inevitably, and the results will by no means match the standardized results * * * The tests will not displace the practical judgment, but may be of great assistance in forming and improving it and even the trained clinical psychologist with the scale at its best, will doubtless have to 'set' it somewhat differently for various social and industrial classes, and will make various allowances for local circumstances, even if not for his own 'personal equation'."

Similarly Yerkes, Bridges and Hardwick: "As matters stand, the

best that can be done with the Binet scale is to interpret the results in the light of such facts as are obtainable. That is, the verdict often depends on the judgment of the examiner almost as completely as when no 'scale' is used." "It is today a matter of common opinion and report that the child's mentality may be measured by almost anyone who chooses to devote a few days to the study of the Binet scale, and to practice in applying it." "But satisfactory mental examination of either the normal or the pathological individual demands thorough training and extensive experience," the necessity for which we have constantly insisted. Is our critic also "inclined to ignore" this "attack?"

Yerkes goes even further: "I believe that the age arrangement of tests is wrong in principle, violating the laws of mental development. For this reason, I have rejected the age scales * * * Possibly it would be wise to ignore the Binet method, and on the assumption that nothing satisfactory exists, to proceed with the development of the sort of thing we think we should have." And yet our critic assures us that "the Binet scale is entirely satisfactory for purposes of mental diagnosis."

The difference, in a word, between, Mr. Kohs' and my own position seems to be this: he is able to diagnose all feeble-minded persons by the Binet scale alone, except between the narrow limits indicated above, while I do not pretend to be able to diagnose with certainty, with all the aids that are at hand, a certain proportion of cases, ranging throughout the greater part of the scale, amounting in one survey to about 13% and in another to about 19% of the total number.

In the second place, I hold that the scale is not as accurate as it has been claimed to be and that it is still capable of substantial improvement. This statement can be demonstrated by an analysis of Goddard's own statistical data and of his relocations on the basis of his announced standard: "We consider that a question is misplaced in the scale if it is not answered correctly by about 75% or more, of those trying it." By "75% or more of those trying it" we have to assume that Goodard referred to 75% of all the pupils of the same chronological age. But "with the purpose of determining just what percentage of children at the various ages can answer the different questions," Goddard distributed the pupils of a given chronological age according to their mental ages in Table II, on which his results are based. In consequence the per cents of successes for a given test (no per cents, however, are given in the table) cannot be calculated for all the pupils of a given chronological age, but only for the pupils

who classify in the Binet age in which the test came in the 1908 scale. Accordingly the number of passes and failures on which the per cents must be based for any given test in any given age is very materially reduced. The highest number of scores for any test is 109, for tests X, 2 and 3 of age 10, while in very many cases it is less than 50. Thus, although there are 114 5-year old pupils, the per cent of successes for the V-year tests can be based only on from 32 to 34 pupils in age 5, or from 18 to 25 pupils in age 6, or from 7 to 10 pupils in age 7. The 5-year old pupils are distributed in the table all the way from age III to age VII. Likewise, although there are 89 13-year old pupils, the percentage of successes for the XIII-year tests can be based only on 5 subjects in age 13, from 24 to 39 in age 12, and from 0 to 12 in age 11. The number of pupils of the chronological ages corresponding to the same Binet ages is as follows for the remaining ages; VI, from 32 to 43; VII, from 96 to 108; VIII, from 74 to 93; IX, from 53 to 72; X, from 105 to 109; XI, from 38 to 53; and XII, from 36 to 44. It is at once apparent from the above figures not only that all the tests of a given mental age were not given to all the pupils of the corresponding chronological age, but that it is impossible to determine what all the 5-year old pupils did in the tests of, say, ages IV to VIII. We can only tell what the 5-year olds who classified as IV did in the Binet IV tests, what the 5-year olds who classified as V did with the V-year tests, what the 5-year olds who classified as VI did with the VI-year tests, etc., and similarly for the other ages. We must bear in mind, then, that the per cents of successes or failures for the tests of a given Binet age are based only on the children of the corresponding chronological age who successfully passed the tests of that age. In other words, the norms established are based on children who had *already been mentally standardized by the 1908 scale*, and not on the 75% standard of successful performance of all the children of a given chronological age. Goddard indeed states that the per cents can be checked up by the results in lower and higher ages—he gives the figures in the table but he does not actually carry through the detailed analysis: "It seemed worth while to determine whether the same ratio approximately would hold good for those who were *above* and *below* age. We have consequently worked out the same problem for those who were one, two and three years above and below age. (This analysis has not been carried through. Only the number of plus and minus are given in the table.) That is to say, if a child is nine years old actually, but tests only six, how does he succeed with the six-year old questions. Have such children on the whole succeeded and failed in about the same ratio as the children who are physically only six

years?" (But this comparison can be carried out only with the six-year olds who classify as VI, and not all the six-year olds). "An illustration will make the problem clear. Take question VI 1. By the children who are 'at age' it is answered correctly by 35 while 8 fail. Of those who are a year above age (i.e., precocious) 34 succeed and 8 fail,³ practically the same proportion. Those who are two years above, 4 succeed and 0 fails, while those who are one year below (i.e., dull), 14 succeed and 3 fail.⁴ This is very close to the ratio of 35 to 8." Whatever may be said in favor of this procedure, it is obvious that the advanced and retarded children who are compared with the children who are 'at age' have also been mentally standardized in advance by the 1908 Binet scale. Moreover, the per cents of successes for children of different chronological ages for the tests in the Binet age in which they classify are often amazingly different, and do not "agree very closely." For example, VI 1: 46.8%, 81.4%, 88.2% and 100%; VII 4, 33.3%, 43.5%, 92.2% and 92.3%; VIII 2, 20%, 48.4%, 84.9%; IX 1, 4.7%, 59.7%, 87.2% and 100%; X 3, 28.9%, 84.4%, 86.6% and 100%; and XI 2, 11.1%, 44.5%, 73.5% and 60%.

When we turn to the actual relocations of the tests made on the basis of the standard adopted, we likewise encounter difficulties. It is not clear just why the five digits test was advanced to age VIII (83.7% for the eight-year olds classifying in VII), as the test proved to be just right for the seven-year olds in age VII (74.7% of passes). The color test is too easy for the seven-year olds in age VIII (95%), but no data are available for the seven-year olds in age VII, where it has been placed. Certainly we are not justified on the basis of the few six-year old cases (14) tabulated in age VIII to decide that the test does not belong in age VI. Let the reader again be reminded that both the seven- and six-year olds considered had a 1908 Binet mentality of VIII, so that we have not come to any conclusion at all as to whether 75% of chronologically seven-year olds would or would not pass the test in question. The rhymes test is nearer the accepted standard for the ten-year-olds in X (72%) than for the eleven-year olds (90%) who classified in XII.

Of tests which were not changed, the patience test proved too hard for V (62.5%). The result for the six-year olds (73.6%) indicates that it should be advanced—the data, of course, do not show what the VI-year 6-year olds did in this test—which seems to correspond with our experience with the test. Counting 13 pennies proved to be too easy for VII (94.8%), and knowledge of coins for X (95.4%), the ages

³In the table the figures are 30 and 4.

⁴The figures in the table are 15 and 17, instead of 14 and 3.

in which they are retained. The six-year VII-year olds passed the counting test (72.5%). The sentence repetition proved too hard for XII (40.5%), where it is retained, as well as too hard for the 13-year olds in Binet XII.

Lest Mr. Kohs should still find the Vineland revision "entirely satisfactory," and lest our analysis should fail to reveal any "very glaring" "statistical pitfalls" in this work—which, by the way, is really statistical—since he is "suspicious not of one, but of all the conclusions" which we reach, we shall allow Thorndike whose competency in statistical analysis he will not question, to speak:

"Goddard has asserted that 'the Binet scale was wonderfully accurate'—that its questions are 'well graded, at least from ages five to twelve, and that they fit the ages to which they are assigned;' and others have accepted this conclusion."

"I shall show that the very measurements whence Goddard derived his assumptions of the exact correspondence of the Binet *mental* ages V to XIII, to the average ability of *chronological* ages 5 to 12, show in reality something entirely different, and that so also do the results of others who have used these tests. * * * The Binet XI fits chronological $12\frac{1}{2}$, the Binet XII fits chronological $13\frac{1}{2}$. You must not test 166 eleven-year olds and only 20 fourteen-year olds and then use the gross numbers of 60 eleven-year olds and only 5 fourteen-year olds testing Binet XI to prove that Binet XI fits the eleven-year olds." * * * "It is clear from Table III (Thorndike's table) that eleven-year old children do not equal Binet XI standard, but a standard of about 10.6 (or X+3-5 of the Binet X-XI step); that twelve-year old children do not equal the Binet XII standard, but about 10.9 (X+9-10 of the Binet X-XI step) * * * Probably 11.4 (or XI+4-10 of the Binet XI-XII step) is as high as the general run of thirteen-year-old children would score." And yet, on the basis of these standards which Thorndike declares fallacious, and on the basis of arbitrary standards of feeble-mindedness based upon them, themselves purely theoretical, we have been assured that "It has been established beyond doubt that 2 per cent of all public school children belong in the class of the mentally deficient" (Maria Dean), that the "Binet-Simon Scale" * * * "has given an accurate classification of their cases, which is fundamental for all special class work," and that, as stated at the last annual meeting of the National Conference of Charities and Corrections by a Pennsylvania field worker who had been trained in an institution to administer this scale and who essayed to diagnose feeble-mindedness automatically by means of it, 98% of unmarried mothers are

feeble-minded! Does our critic intend to imply by his initial statement that clinicians should continue to "ignore attacks upon their methods," as indeed some have attempted, arrogantly but unsuccessfully, to do in the past, as if these methods, sacredly authoritative, are inviolable and immune to criticism?

I believe that every unprejudiced student must admit that, no matter how valuable the Vineland revision may be, it has thus far not been "mathematically demonstrated" that it is a "marvel of accuracy" and that it will "tell us to a nicety just where a child stands in his mental capacity." All we are justified in saying is, that it possesses a *fair* degree of accuracy and that it is of considerable value for the estimation of the intelligence level—whether more valuable than Yerkes' point scale has not yet been demonstrated. It is not a question as to whether the instrument is "fool proof," but whether it possesses the degree of accuracy constantly predicated for it, and whether it can carry the large diagnostic burden that has been injudiciously imposed upon it. We should not be deluded into believing that the scale is not susceptible of a very marked degree of improvement, both as to the relocation of some of the tests, and as to the substitution of better tests for the many poor tests which it still contains.

In the third place, even if the scale were *absolutely* accurate that would not prove that the proposed arbitrary standards of measuring feeble-mindedness by means of it are correct. There is no apriori certitude or sacredness about the theoretical standards which have been proposed, nor is there indubitable experimental warrant of their accuracy, which is amply demonstrated by the fact that those who have proposed fixed standards have been constantly hedging, in response to criticisms, so that the standards originally proposed have now almost been relegated to the limbo of forgotten exotics. In the previous sections of the chapter we discuss (1) the proposed arbitrary retardation standards, (2) the "intelligence quotient," and (3) the XII-year standard of feeble-mindedness. It is not necessary to advert to the first of these since they are not referred to in the critique.

Whether the intelligence quotient is "generally used" is immaterial to the argument. It has been persistently recommended as a markedly superior substitute for the 2-, 3- and 4-year retardation standards. All "those who do use it" do not "confine themselves to the lower ages," if by "lower ages" is meant less than 20. Possibly it has been used beyond that age, just as the adult years have frequently been used for determining the absolute difference (even by Huey). I am quite well aware of Stern's suggested limitation of the

quotient to the "period when the development of the feeble-minded individual is still in progress." According to Simon, and Stern as indicated later, that would be at the age of IX,⁵ and according to Mr. Kohs about X³ according to his upper standard of feeble-mindedness as formerly given (The Practicability of the Binet Scale and the Question of the Borderline Case), but "certainly not later than age 13," according to his present pronouncement.

Since Stern does not think that the quotient is applicable to idiots, or even to those whose mental development has ceased, it is evident, that if he were followed the intelligence quotient would not have the great value that has been claimed for it. It would be inapplicable beyond IX (and not "13" as our critic intimates) and could not be applied to feeble-minded individuals whose "development" has stopped. But there are many feeble-minded individuals whose development is so slow that it is extremely difficult to say whether their mental growth is advancing or has stopped. Certainly the examiner cannot from a single examination and by using the intelligence quotient determine this with any accuracy in many cases, hence at best the quotient could only be used in a most tentative way. We do not believe, however, that the maximal divisor should consist of the upper mental age limen of feeble-mindedness—which Stern has placed lower than the proposed mental ages of the "middle" and "high" grade morons. If we are at all justified in using an artificial maximum—experimental data which we have ready indicate that the quotient cannot be used even during the teens without the employment of a corrective or attenuating formula—the divisor should not be less than the age of mental maturity of the normal individual. On this theory, however, we render the intelligence quotient impotent for present use, because nobody now knows what this age is, for it has never been scientifically established, nor do I know that it can be established. The ages of 15 and 16 have indeed been proposed but we shall have need for more evidence of the correctness of these hypotheses before we can accept them. My own results with the Seguin form-board (partly in print) and with 12-intelligence tests indicate that many of the traits improve up to the highest age tested, 17. Nor am I justified in inferring from the experimental data that the increase does not extend beyond 17. The presumption is that some traits mature at 15, others at 16, 17, 18 and some probably

⁵It is not without significance, as we shall see later, that Mr. Kohs adroitly omitted this very pregnant sentence from his quotation from Stern: "In the case of the later age-levels this is easily intelligible, for once the stage of arrest that we have previously discussed is reached (for morons at the mental age of 9), the quotient obtained by dividing mental age by chronological age increases."

after 20. At the present time, therefore, there is no divisor which we can employ for the adolescent or adult years except the chronological age, the employment of which lands us in absurdity. It was exactly this which I aimed to show in the latter part of the chapter where the quotient was considered.

However, if we use the proposed age 16 as the highest chronological age for our adults—and from what I have said I do not countenance this procedure—it will be found that everyone of the subjects except one grades as feeble-minded according to the “three-quarter standard,”⁷ so that the conclusion is almost the same as if the actual chronological ages were used, except that the subjects would all rank among the higher grades of the feeble-minded.

4. Mr. Kohs has given no facts which invalidate my central point, that we cannot diagnose or classify persons en masse or a la wholesale as feeble-minded, because they fail to reach Binet XII, the standard which has been most frequently followed and still is followed, or even because they fail to reach XI or X. I do not hold that the proposed arbitrary XII-year standard, or even the modified XI-year standard, is automatically “applied in any laboratory making claims at cautious, accurate, scientific work,” but I do affirm without fear of contradiction that many statistical surveys based on the Binet tests of older adolescent and adult criminals have been made purely on the basis of this standard (or on the basis of the 2-, 3-, or 4-year standard of retardation), and I give facts to substantiate my position in the earlier part of the chapter. Moreover, innumerable individual diagnoses have been made purely by these standards in the courts and schools of the land, and children have been assigned as feeble-minded to special classes on the basis of these arbitrary standards. I know this to be a fact from a personal inspection of the work in many school systems. It is only infrequently, of course, that the authorities will state this baldly in print. But I shall quote from a document published from the sixth or seventh largest school system in the country, published as recently as February, 1916: “The children are assigned to the class (speaking of the “subnormal” classes) through the psychological clinic (in charge of a Binet tester). They must show a mental retardation of two years below the age of nine, or three years above the age of nine, and must also be pedagogically retarded.” “Prevocational classes enroll children over fourteen years of age who are thought definitely to be feeble-minded.” In one of these classes I found one child of Binet XV, three of XII, eleven of X and five of XI.

Mr. Kohs himself has recently announced his rejection of the

⁷All are feeble-minded by this standard when the “improved” scores are used.

XII-year standard. He has brought the standard down to 10³ and he says that other students "have arrived at almost the same conclusions regarding the lower limen of borderlinity." He has, however, provided himself with an elastic zone extending from 10⁴ to 11² which he calls the "borderline" zone. In the ultimate analysis, he absolutely abolishes his zone of borderlinity, for in his final diagnosis he gets rid of all of his borderline cases by pronouncing the majority of them "feeble-minded" and the rest of them "normal." We take no issue with Mr. Kohs in his attempt to lower the upper limen of feeble-mindedness. On the contrary, this is precisely the point on which we have insisted. However, we have brought the standard about three points lower than he has, having provisionally placed the limen somewhere between the ages of IX and X. We have arrived at this tentative conclusion as the result of the analysis of our own data covering a number of years. Since, however, Mr. Kohs regards our own data as "prejudiced" and "perverse," and openly challenges their "comparability" and accuracy from the "scientific viewpoint," it is comforting to be able to present to him corroborative testimony from authorities whose scientific competency he will probably not attempt to disparage.

Simon tells us that "We mean by the term 'idiot' an individual whose intellectual development is that of 2 years or under. We mean by the term 'imbecile' an individual whose intellectual development corresponds to that of a child between the ages of 3 and 7 years. And finally, by the term 'feeble-mindedness' (debile) we mean an individual whose intellectual level, while superior to that of a child of 7 years, is nevertheless below the average development of an adult. This latter degree of development, I have said, is still inadequately determined. Provisionally it may be proposed to fix at 9 years the upper level of mental debility." We have already quoted Stern to the same effect who, moreover, also cites Binet as holding "that the imbecile does not progress beyond the mental age of seven, the moron (feeble-minded in the narrower sense) beyond the mental age of nine." Stern evidently adopted the term moron from American usage, and added the parenthetical clause to make the word correspond to Binet's usage. It is evident that neither Simon nor Binet provided a place in their classification for the "middle" or "high grade morons."⁸

Neither does Hill appear to provide a place for these categories, for in his classification of 63 delinquent and destitute boys, all of those

⁸Simon's statement was made as late as the fall of 1914 and therefore supercedes one or two earlier statements made by Binet and Simon in which the influence of the American recommendation could be seen.

classed as morons had a mentality of less than 9.6 except one. In giving the results of the examination of 195 children Porter, Huffaker and Ritter state that the "morons are such high grade feeble-minded as never at any age acquire a mental age greater than 10 years"—how about the 11² here? M. Hamblin Smith examined 80 consecutive male and 80 consecutive female adult prisoners. Of the 64 classified as non-feeble-minded 21 did not reach a mentality of X years and of these three failed to reach IX. None of his feeble-minded succeeded in the X-year group, while only 23% passed all the tests in the IX-year group. "I have found very marked deficiency of intelligence in subjects whom I am sure no observer would think of calling feeble-minded, still less of recommending their detention as mental defectives." And yet, our critic assures us that the "existence of mental deficiency" is demonstrated by the "Binet scale itself" in "a subject testing 10³ or below!" It is very evident that Mr. Kohs' views are not shared by all workers who are authorized to speak in this field and that the views which I represent are not "unfounded," simply because he so regards them. The objection that these views are "shot through with such fundamental errors that the whole thesis is invalidated and thrown out of court," will scarcely avail against all others who have arrived at similar conclusions.

5. Mr. Kohs assumes that I know nothing about the theory of overlapping and that I embrace the postulate of a perfectly inflexible arbitrary intelligence standard of feeble-mindedness. He imagines that I hold there is a sharp line between the feeble-minded and the normal, while the fact is that I neither now nor have I ever at any time embraced such a theory. In fact, I have insisted that there is a continuous gradation of mental deficiency. This is amply shown by my diagnoses (See *The Mental Health of the School Child*, p. 147). I recognize the need of, and have always used, a zone of borderlinity. My point of disagreement with Mr. Kohs is as to his ultimate disposition of the borderline cases. The fact is that he has no borderline cases, because he reclassifies most of them as feeble-minded and all the rest as normal. He is able to classify all his cases from the standpoint of intelligence into two groups, "feeble-minded" and "normal," while I am unable to get along with less than 7 or 8 categories and Healy with less than a dozen. A certain number of my borderline cases are eventually classified as feeble-minded (but frequently only after months or years of observation), some of them remain as borderline, others are classed as backward, while rarely is anyone classed as normal. Moreover, I recognize that there are mentally short individuals whom we may be justified in regarding as socially in-

competent, based on a repeated record of social breakdowns, although they are not actually feeble-minded intellectually considered—a class of cases to which Hickson seems to have referred as “sociopaths.” From the tenor of Mr. Kohs’ remarks, however, I infer that he would diagnose the same individual as feeble-minded (intellectually speaking) if he were socially delinquent but as “normal” if he were law-abiding. If he were dealing with a group of borderline criminals he would probably call most or all of them feeble-minded, while if the group consisted of successful law-abiding people of the same B.-S. ages they would probably all be rated as normal. It is probable that the scales in the two cases would be turned by social considerations rather than by “other tests.” We, on the other hand, would probably diagnose as intellectually feeble-minded a limited number of the first class, and a certain number as social incompetents (assuming that this had been proved by their records) who intellectually are borderline or backward or retarded, while we should diagnose the members of the second class as borderline or backward, as the case might be.

It must be perfectly obvious to any one experienced in mental diagnosis that the diagnosis of feeble-mindedness in the borderline zone is largely a matter of the “personal equation” of the examiner. If the examiner is lenient in his standards and is less concerned about the exact mental status of his subjects than their record of social delinquency he will find an easy excuse for finding an enormous number of feeble-minded persons. In fact, some examiners have reported almost all of their examinees to be feeble-minded. On the other hand, if the examiner is conservative, and gives considerable heed to the intelligence level of his cases, he will find a much smaller percentage of feeble-mindedness although he may find a larger number of social incompetents for other reasons. It would be easy to illustrate these facts, were it not invidious to do so here, by comparing the diagnoses made by different examiners in the same city and in different cities dealing with cases more or less similar in respect to the nature of the offense and of the social, economic and environmental conditions, and of approximately the same or not largely differing ages. The differences in the percentage of feeble-mindedness returned in such surveys are astounding, varying from about 5% to more than 85%. Different examiners in the same city have made markedly different diagnoses of the same cases. Concrete instances of this kind were reported to me immediately before and after the Chicago paper was read. The amazing discrepancies in the intelligence diagnosis of different examiners must give us pause. They will have to be explained by more satisfactory considerations than the

happy inventions which have been proffered during the last few years. Psychological diagnosis has not yet been brought to the point where it is safe to say dogmatically, as is being said constantly, that the majority of criminals are feeble-minded. There is still need of sounding a conservative note lest not only "the general public" but we who are examining be also "led astray."

6. It is evident from what has been said that there is no justification for the comment on "Dora M." "According to Wallin's arbitrary standard she should be considered normal and consequently *forced* to be at large!" My "arbitrary ten-year standard" is not arbitrary in the sense that it is a perfectly rigid and fixed criterion which can be applied indiscriminately to all cases—since I have denounced this assumption repeatedly⁹—and I do not regard an adult with 10.5 mentality as "normal," even did I not regard her as feeble-minded. It is evident, however, that Huey's judgment as to the social inefficiency of the cases cited was not based purely on their intelligence status. He expressly concedes that "David F." has sufficient capacity to "support himself in society as a contented shoemaker, would help to entertain the community with his music, and would be made to live the life of a useful if humble citizen." He does not dogmatically assert that the boy is "feeble-minded without the least possibility of doubt," nor even that he is below the line of feeble-mindedness, but merely that he is "very little below the borderline of feeble-mindedness." Moreover, on p. 161 he classified 28 of his 35 subjects as "border cases." Neither these cases nor the cases cited from Goddard in any way invalidate my position that there are adults in society who do not exceed X or XI years mentally whom we are not justified in calling feeble-minded simply because they do not exceed these Binet ages. And yet, less than three months ago this unqualified statement was made: "A man thirty or forty years of age who has a mentality of a child of ten or eleven, is thus not only a helpless individual, but he is positively harmful. If he does not show his harmfulness it is usually because he is extremely helpless or because he has been protected and cared for by his relatives or friends or in an institution." By this standard everyone of our thirty and forty year old successful farmers would be "helpless" and "harmful," and in need of "protection."

I have records of persons committed to institutions as feeble-

⁹As I have frequently stated elsewhere in this book, my attempt to apply arbitrary quantitative standards of intellectual retardation in the diagnosis of the various cases coming to a university clinic in a populous district has, in the main, proved quite futile—The Mental Health of the School Child.

minded who are now at large and achieving success in life. A recent case, diagnosed by the superintendent of a state institution as feeble-minded on the basis of the Binet tests (XII-year standard) got away from the institution in spite of the superintendent's determination to retain him. He is now in a preparatory school, at the head of his class, ready to enter college in the fall. How many non-feeble-minded children committed to state institutions are permanently retarded by the limitations of the institutional routine no one can say.

7. Just what "very glaring" "statistical pitfalls" lie concealed in my selection of a few normal cases, some of whom had had the advantages of good schooling and others of whom had had meager school training, but all of whom were successful as judged either by socio-industrial criteria or by school progress, the critic fails to elucidate. In the first place, I did not select the two groups of cases in order to "demonstrate the influence of school training," but merely in order to determine how the XII-year criterion of feeble-mindedness would "stand up" when groups of adolescents and adults, none of whom could be considered feeble-minded, were tested by the Binet scale. I did not attempt to carry out a "statistical" study. Had I done so, I should have selected more cases. That the two groups differed considerably in chronological ages was entirely immaterial for the purpose I had in view. A group of younger, poorly trained individuals who had had time to demonstrate their capacity to make a living would have served just as well. Moreover, let me emphasize that the age distribution in the older group did not differ essentially from the age distribution of penitentiary convicts, nor did the age distribution in the younger group differ materially from the age distribution of prostitutes and delinquent boys, who have been classified as feeble-minded *purely on the basis of Binet surveys*, and in which absolutely no account has been taken of any possible hypothetical or actually demonstrated dementia. I can recall only one Binet survey which has made "allowance" in the actual published tabulations for mental decline—persons of all ages have been tabulated as aments by the Binet scale—although I have no doubt that there are cautious examiners who distinguish between amentia and dementia.

Our critic's smiling objection to our two older cases, however, is quite immaterial (Of course, we do not concede that they were appreciably demented much less in their "dotage"). Every one of the group who were in their prime proved feeble-minded when measured by the XII-year standard which has been widely followed for over five years. Moreover, one of the younger subjects graded practically the same as the two older ones—indeed lower when the "improved

form" of the scale is used! Mr. Kohs says that "no competent Binet examiner would call Mr. A (65 years old) and Mrs. B (59 years old) feeble-minded in view of all their previous history." That, of course, is exactly the point we have been laboring to make, namely that you cannot call an adult feeble-minded simply because he does not grade beyond X mentally. What we object to is the exaggerated tendency to attribute all or most cases of delinquency to "mental deficiency." If a given X-, XI- or XII- year old adult or older adolescent is delinquent, then he is "feeble-minded without the least possibility of doubt," but if another X-, XI- or XII-year old is respectable, law-abiding and successful, then he is "normal." We insist that the rule must work both ways—of this more anon. Had the ages of our older cases been "19 and 17 and tested 10³, a totally different story would undoubtedly have to be told." No doubt, Mr. Kohs would have diagnosed them as feeble-minded by the Binet alone, and recommended their permanent commitment to a feeble-minded institution, while we, mindful of their constitutional rights to life, liberty and the pursuit of happiness, would have given them the benefit of the doubt and afforded them the chance to live the lives which they have proved their right to live. We do not believe in depriving individuals of the rights guaranteed them under the constitution, unless they have forfeited such rights by reason of their criminal propensities or by reason of the fact that they are incapable of exercising such right through social incapacity. Moreover, we insist that every individual must be presumed to be socially competent until it has been proved beyond a reasonable doubt that he is not socially competent, while we likewise insist that persons who have been proved to be socially irresponsible by a critical consideration of all the facts bearing on the case should be treated as irresponsible rather than as criminal.

The position we have taken with reference to the establishment of the upper limen of feeble-mindedness is precisely the position Binet has taken with respect to the bearing of heredity (see p. 59 *supra*). We cannot determine the upper limen by confining our study to abnormal, pathological institutional material as our critic has done (10³). A limen thus established does not mean anything until it has been subjected to a check test. The only valid check is to test adolescents and adults who are poorly trained as well as well trained, poorly envired as well as well envired, who are at large in society and who are making good. Our critic has presented no proof to show that the principle announced on the first page of our paper is false. On this question we prefer to stand with Binet as against our critic. Moreover, Bronner, by a somewhat similar method of comparison, has reached

conclusions quite in harmony with our own in respect to the prevalence of feeble-mindedness among delinquents.

8. It is evident that Mr. Kohs in his critique of my criticism of "Binet testers," uses the word in one sense, while I use it in a wholly different sense. He seems to apply the term to any one who uses the Binet tests, no matter how well or how poorly trained or experienced he may be in psychological diagnosis, while I restrict the term, and always have so restricted it, to teachers, nurses, social workers, or any other class of workers whose chief equipment for the work of psychological diagnosis is that they have learned to give the Binet tests,¹⁰ and possibly a few other tests. I have referred indifferently to such workers as "Binet testers" (because they almost invariably rely on the Binet tests) or psychological "amateurs," and have taken the position that, while Binet testers may be qualified to give the Binet tests, if properly trained, they are not qualified to make psychological diagnoses. Mr. Kohs has presented no fact to prove that this position, which I have been defending for half a decade, is "shot through with such fundamental errors that the whole thesis is invalidated and thrown out of court." The fact is, the position I take is substantially the position of Binet himself and of other experienced psycho-clinicians. Since Mr. Kohs has grown "suspicious not of one, but of all the conclusions" reached by me, I shall be obliged to quote again:

Binet concludes with a final warning: "A last word for those who desire to employ the method. Any one can use it for his own personal satisfaction or to obtain an approximate evaluation of a child's intelligence, but for the results of the method to have scientific value it is absolutely necessary that the individual who uses it should have served an apprenticeship in a laboratory of pedagogy or possess a thorough practical knowledge of psychological experimentation."

Likewise, Goddard, who has defended the use of the tests by teachers, parents, and "even novices" and "untrained or wrongly trained persons," who has affirmed that "Binet's plan is perfect," who has said that "by the use of this method, any superintendent may examine his system of schools, and determine with considerable accuracy whether each and every child is in the grade that his mental ability actually warrants," and who has said that the "criticisms that from time to time appear, arouse in those familiar with its use a smile and a feeling akin to that which the physician would have for one who might launch a tirade against the value of the clinical ther-

¹⁰Mr. Kohs' accusation, "What perversion of the facts for the critic to place all mental testers, with perhaps the exception of one or two, in the same category with the inexperienced, the poorest amateur," is itself a flagrant perversion of the position I take.

mometer," has nevertheless reached the following conclusion: "Does it Require Highly Trained Experts to give the Tests? Yes: To determine a child's mental development to a nicety with a degree of precision and a certainty that cannot be doubted, requires the highest kind of training on the part of the person who gives the tests. Such a person must be a good psychologist, must understand the human mind, must understand the methods of scientific procedure * * * * and finally must know how to interpret the answer that the child gives and give to it the correct amount of credit. Besides a general knowledge of psychology and of child psychology in particular, this involves considerable experience with the scale itself."

So also Mitchell: "In a corresponding way the Binet-Simon tests may be and are being conducted in Cleveland and elsewhere by examiners who have thoroughly mastered this special technique, but who are not trained psychologists and who are not able to administer the other available tests in the same field. Competency in this field involves ability to use a wide range of psychological tests and measures and a grounding in the theory and practice of applied psychology." "Diagnosis of mental status should be made by a clinical psychologist." "Some psychologists are concerned with the behavior of normal human beings. They never carry their observations beyond the confines of the laboratory psychology. These people are in the same relation to the clinical psychologist as the experimenter in the medical laboratory is to the practicing physician," a position which I have persistently maintained.

Moreover, the American Psychological Association has, by special resolution, gone on record in this controversy:

"Whereas, psychological diagnosis requires thorough technical training in all phases of mental testing, thorough acquaintance with the facts of mental development and with the various degrees of mental retardation:

"And whereas there is evident a tendency to appoint for this work persons whose training in clinical psychology and acquaintance with genetic and educational psychology are inadequate:

"Be it resolved, that this Association discourages the use of mental tests for practical psychological diagnosis by individuals psychologically unqualified for the work."

Does our critic regard the conclusions of this association as "impregnated with prejudice," and as intended to "lead the general public astray?" The stand which I have consistently and persistently maintained, in the face of criticism that has often descended to the personal, is the recognized authoritative position. The only

point which I have emphasized more than anyone else is that the psychologists themselves must supplement their theoretical training in psychology and education by extensive first-hand studies of various mental types, by a specialized study of the problems of mental deficiency, and by a certain minimum of medical training. If Mr. Kohs differs with me on the positions which I really hold—and not on the positions which he intimates that I hold, but which I repudiate—I have no quarrel with him, but I do not follow him. If he holds that no classifications and diagnoses are being made by Binet testers, in the sense that I use that term, I differ with him again. I will go further and assert that the majority of the mental classifications in the schools are still made by teachers purely on the basis of the Binet rating. If, unfortunately, it is necessary that this must be so we must clearly recognize, and have no hesitation in clearly stating, the limitations of such work.

Far from having “prejudiced” and led the “general public astray,” the “attacks” which we have made, in common with other writers, during the last four years have probably in no small measure contributed to the demand for the preparation of “competent psychologists,” in the clinical work, have stimulated the spirit of “cautious conservatism,” which is so urgently needed in this field, and have not “retarded real progress,” as our critic fain would have us believe, but have actually promoted it. There have been, and still are, tendencies in the field of psychological diagnosis which distinctly need curbing. As a profession we must disabuse ourselves of the idea that no one can do good work in this field who does not report immense percentages of feeble-mindedness, and that anyone who has learned to give the Binet and who has spent a few weeks in an institution for the feeble-minded can be drafted as an “expert on feeble-mindedness” (I am quoting the exact phraseology) by state and national commissions to make reliable, worth-while surveys of feeble-mindedness. Surveys so made may indeed furnish interesting and stimulating reading—their sentimental and propagandist aims are usually only too obvious—but what value have they as scientific documents, purporting to give accurate mental diagnoses of the populations which have been surveyed? When the most competent and experienced authorities on the feeble-minded freely admit that they are frequently unable to tell whether an individual is feeble-minded or not, shall we permit and actively encourage the making of mental classifications and surveys of feeble-minded persons by amateurs who are willing to diagnose by rule-of-thumb recipes? It cannot be admitted that we have arrived at a point in psychological classification where it is

good public policy or good science to attempt to estop criticism designed to make us scrutinize more closely our own methods or to awaken us out of the dogmatic slumbers into which we are so prone to fall as soon as a slight advance is made in this field.

The verdict of science has not yet been rendered for all time to come, and no one person or group of persons have already given the final solution to the problems in this field. Time will mitigate the severities of our critic's reflections.